1.9.4.1

UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
REGION 10
1200 Sixth Avenue
Seattle, Washington 98101

Reply To
Attn Of: OEA-095

May 10, 2004

MEMORANDUM

TO: Dr. D. Wayne Berman, Aeolus, Inc.

FROM: Julie Wroble, Office of Environmental Assessment, Risk Evaluation Unit

CC: Dan Heister, Office of Environmental Cleanup, Oregon Operations Office
Dr. Pat Cirone, Unit Manager, Office of Environmental Assessment, Risk Evaluation Unit
Dr. Aparna Koppikar, National Center for Environmental Assessment
Richard Troast, Office of Solid Waste and Emergency Response

SUBJECT: Comments on the Draft Soil Sampling Results and Preliminary Risk Assessment for the North Ridge Estates Site, Klamath Falls, Oregon (March 23, 2004)

Overview

Attached please find my initial comments on the above-referenced document. In general, this document is much improved over the previous version (November 21, 2003). Dr. Berman has addressed most of my comments and included additional details on the emissions and exposure modeling performed, the values selected and rationale for their use, and site-specific information that was considered. In addition, the approaches described for modifying the AP-42 dust models to be applicable to this site were clarified, but I would like additional assistance in reviewing the dust models.

Another issue that should be addressed is the discrepancy between EPA's "acceptable" risk range and Oregon DEQ's soil cleanup rules, which state that individual carcinogens in soil must be cleaned up to a level posing no greater than a 1 x 10-6 excess lifetime cancer risk (OAR 340-122-045).

Please contact me at 206/553-1079 if you have specific questions on these comments.

Note: I am making the changes required to address the current conditions in the field. However, when combined with your suggested changes to the calculations, I do not expect risk estimates to change radically (at least for the pathways already addressed). Some of the new pathways, however, may affect things more radically. Thus, there will be some additional recommendations in the revised soil report.

Specific Comments

Page 1, 2nd bullet: What is the basis for the statement: "Even if the remaining ACM were to completely degrade, the resulting asbestos concentrations in the soils would not be adequate to pose an unacceptable risk." I don't know that sufficient sampling and testing of soils has been completed to support this statement.

This statement is based on a calculation. It does not depend on the number of samples

collected. However, I inadvertently forgot to substitute the newest value from the calculation into the document. The correct value is 0.3% not 1%. I will make a global change throughout the document to correct this error.

Page 1, 4th bullet: What constitutes occasional handling of pieces of ACM? Should this statement be qualified to discount handling of weathered material?

Occasional handling means, for example, picking up pieces occasionally encountered to place them in a plastic bag for disposal. As long as they are handled carefully, it should not be a problem occasionally handling ACM, even if it is weathered. Actually, the text of this section will be changed to reflect additional consideration of amphiboles.

Page 1, 6th and 7th bullets; also page 4, 3rd paragraph; : Including the caveat about "required dust suppression activities" may not be acceptable to residents because of their prior experience with the developer, who did not comply with an EPA order for proper disposal of ACM. Further, such institutional controls cannot be considered as part of the risk assessment; however, a recommendation for dust suppression can and should be made.

There are three points that need to be considered in response to this comment:

First, "required dust control" refers to routine requirements associated with control of nuisance dust not anything having to do with asbestos. In the main body of the text there is a reference to the nuisance dust standard to make this clear. However, I felt that discussing the nuisance dust standard in this section is unnecessarily detailed for the executive summary.

Second, in the sixth bullet, the indicated phrase is not a caveat. As it reads, the risks are acceptable whether or not dust control is practiced. I simply made the additional observation that, if required dust control is practiced, risks will be even lower.

Third, the seventh bullet refers to worker exposure and, therefore, is more typically handled under OSHA regulations (which involves comparison with exposure concentrations to the OSHA PEL). However, because this is beyond the scope of this document, which focuses on residential exposures, I did not make the formal comparison. Rather, I indicated that risks to workers will be acceptable as long as they maintain the required control of nuisance dust. I believe that this is fairly standard language for the worker pathway in a residential risk assessment. Certainly, I have used it countless times in the past (and not just for asbestos).

Actually, the wording in this section has been further modified to reflect amphibole asbestos considerations.

Page 2, first complete sentence: Add the word "visible" prior to "separated ACM."

Although there may be places in the document in which it is important to indicate that ACM was separated visually (and this is indicated in the main body of the text, as appropriate), I do not think it adds anything to describe ACM samples collected for analysis as "visible" ACM. I am not even sure what such a modifier might mean in this context.

Page 2, 2nd full paragraph: The last sentence in this paragraph implies that amphibole asbestos has been evaluated quantitatively in the risk assessment; however a less formal, qualitative assessment was

performed. Some additional quantitative assessment of amphibole asbestos should be completed because in some areas of the site, amosite is present and has been detected in multiple media (e.g., air and soil).

I will be modifying the later sections of the report to account for the latest findings in the field vis a vis amphiboles. The indicated paragraph simply states that both chrysotile and amphibole are considered, which I believe is the correct and appropriate statement for this executive summary. Actually, the modified report addresses amphibole asbestos explicitly.

Page 2, last complete sentence: Although the cut off appears to be around 1% ACM mass in soil, there is a sample where ACM comprised about 0.2% and asbestos was found in soil. Are there enough data to make this conclusion about the relative mass fraction of ACM? For areas where removal of ACM has already occurred from the soil surface, additional cleanup of the remaining soils may be warranted because the ACM may have released fibers in sufficient quantities to be releaseable from soils. How this is achieved requires careful consideration and implementation.

There are two parts to this comment:

First, regarding the one sample to which you refer, only a single, chrysotile structure was observed in the soil component of this sample (which is why the reported concentration for this sample is right at the analytical sensitivity for the analysis). Given the limitations of asbestos analysis, observation of a single structure is not statistically significantly different than observation of zero structures. In fact, as I discuss in the text, this is one purpose for pooling results — one should not automatically assume that observation of zero structures suggests absence of asbestos. Given such considerations, I do not think that my statement that, generally, samples less than 1% do not show detectable concentrations of asbestos.

Second, the composite samples that were collected at the site reflect the conditions in both soils and ACM that existed at the time the samples were collected, which is still subsequent to earlier removal actions (although it is prior to the most recent removal action). Therefore, estimates of the total asbestos content of these soils at that time (based on the combined contributions from the soil component and the ACM component) represent the maximum possible concentration of asbestos left in these soils. Moreover, because the ACM component was removed immediately after sampling, the concentrations of asbestos in the soil components represent the current residual concentrations of asbestos in soils at the site, absent ACM. Thus, except for the new ACM that has surfaced due to freeze-thaw (which I am currently evaluating), "special" handling of soil does not appear to be required.

That new ACM could be driven up through these soils by freeze-thaw was not unforeseen (as early discussions and at least one of the work plans we submitted shows). That is also why we discussed evaluating the depth of ACM contamination in these earlier documents. Note that the current soil report was not designed to address these additional issues, it was designed to interpret the data already collected. However, the findings of the current soil report can be applied to these additional issues. Thus, for example, if an efficient manner can be found to ascertain the mass fraction of ACM in soil samples, this could be applied rapidly throughout the site (horizontally and vertically) to identify those areas of the site where reservoirs of asbestos-containing material needs to be reduced or sequestered.

Page 3, bullet list: ***Consider adding "riding ATVs/4-wheelers" to list of modeled exposures. This***

activity has been observed on site in areas where ACM has been present. EPA is considering doing task-based monitoring of this scenario this summer.

This will be done.

Page 4, second paragraph: If amphiboles are identified in any samples that EPA is analyzing, then additional evaluation of amphibole asbestos at NRE should be performed. EPA would like to see the raw data for all samples, particularly the sample in which amosite was detected.

The raw data will be provided for all of our analyses.

The EPA data has been formally incorporated into the revised report

Page 5, first bullet: ***Perhaps a physical handling/abrasion of ACM scenario could be performed in the glove box to measure release of fibers from such an activity. I expect that our glove box and elutriator results may help truth some of the modeling that Dr. Berman has completed.***

I do not see how the glove box results can be helpful here. All that the glove box can indicate is whether asbestos structures can be released during handling of the ACM and we already know that. Because there is limited air circulation in the glove box, concentrations measured in the glove box do not relate in any direct way to actual concentrations that might otherwise occur during exposure in the field. Moreover, any concentrations measured in the glove box will be dependent on a subjective decision concerning how long the activity being studied is conducted. In an enclosed space with limited air circulation, airborne concentrations will continue to increase as long as generation of asbestos continues. This is nothing like what happens in the field, where concentrations reach a steady state that is a function of the rate of generation and independent of the duration of generation.

Page 5, remaining bullets: These are excellent recommendations for additional characterization and remediation at the site. However, note that asbestos has resurfaced at several locations across the site. Therefore, a more permanent remedy may be needed at such locations to prevent future resurfacing of ACM.

Thanks! Your second point is also noted.

Page 11, first paragraph: Please use more explicit definitions of the various fibers listed in the table headings. I assume that no fibers shorter than 5 microns in length are represented in the table, but that should be explicitly stated. The second paragraph on this page alludes to that, but the table includes a total structures column which is unclear. EPA would like one copy of the raw data sheets.

I understand the confusion: short ISO structures are different in length than short protocol structures. I will update the table and the document to make this clear.

Page 12, 3rd paragraph: Dan, can you provide me with more information about the nature of hot spot 6? See figure A-2.

I would be interested in hearing whatever Dan knows about HS-6 as well.

Pages 13 - 17: Excellent discussion and description of the statistical evaluation performed for soil samples.

Thanks!

Page 18, Section 4.1.4, 4th paragraph: Given the conclusion that asbestos concentration associated with hot spots are elevated, how will additional hot spots be identified? Where removal has already occurred, what additional remediation of soil in the hot spot vicinity is warranted? What can residents do if new hot spots are identified or emerge over time?

I have modified this discussion in the report and will provide a preliminary definition for a formal definition of a hot spot that should facilitate future identification. Currently, hot spots appear to have been defined based on observably elevated levels of ACM in the soil.

Page 20, 3rd paragraph: I'm not sure I follow the logic in this paragraph. For the ACM component of each sample, was the ACM completely broken down so that all fibers were released? I know that the ACM was cut into small pieces, but would this liberate all the asbestos? Some may remain in some of the larger chunks remaining in the sample. So, in fact, the potential release of fibers in the future could be greater. This uncertainty should be described.

Unlike chemical analyses, no bulk asbestos sample can be "completely" broken down because grinding the sample sufficiently to reasonably assure complete liberation of fibers can destroy the asbestos itself. Therefore, all other bulk methods incorporate procedures for sample preparation that were developed ad hoc and tend to be quite subjective so that they are not reproducible. In fact, other than my method (the Superfund Method/Modified Elutriator Method), there is no other codified or standardized procedure that has been published for the preparation of soil samples for the determination of asbestos.

My method incorporates and alternate approach for assuring that we can adequately account for all of the asbestos in a sample. I use respirable dust as an index of the degree to which the sample has been degraded so that, by taking the ratio of asbestos to dust, this fraction accounts for the degree to which the asbestos that is measured reflects the concentration of asbestos in the entire sample.

The background section of the Modified Elutriator Method presents the results of a study demonstrating that the above-described approach for adjusting the asbestos observed during measurement to account for asbestos in the entire sample in fact works. In this study, three samples were analyzed in the standard manner. These three samples were then ground and re-analyzed. Comparing the results from the ground and unground samples show that my method provides the same measured concentration of asbestos whether the samples were ground or not. This would certainly not be true for any other bulk asbestos method.

The conclusion from the above is that my method provides a measurement of the concentration of asbestos that is an inherent property of the sample (much as concentrations determined from a chemical analysis are inherent properties of the sample) and the measurement is largely independent of the manner in which the sample is handled or manipulated prior to analysis. Certainly, my method does a much better job at this than any other asbestos method currently available.

Given the above, the bulk measurements presented for Klamath Falls should be considered to represent the total concentration of asbestos in the entire sample (for all samples). Thus, if we account for all of the asbestos observed in the measurements from the ACM component of the sample, this is equivalent to how much asbestos would be released to the soil if all of the ACM were to completely degrade.

As a final consideration, a qualitative "reality" check on the above statements can be obtained by comparing measured concentrations of asbestos in ACM samples obtained using my method to the "best estimate" conversion factor for PCM fibers to mass from the literature: 3 x 10^{10} f/g (see Asbestos Health Effects Assessment Update, USEPA 1986). In comparison, we are reporting concentrations that range up to 2 x 10^{9} (6 x 10^{9} for protocol structures), which suggests a mass of asbestos of 10 to 20% of the mass of the sample. Given, (1) that we are not including the largest asbestos assemblies (which contribute the most to asbestos mass) when we use the counting rules adopted in our study and (2) that formulations for many of these materials were on the order of about 10 to 20% asbestos in any case. It seems likely that we are seeing all of the asbestos that there is to see.

Page 20, 4th paragraph: This paragraph describes the data presented in Figure 1. In this figure, has the outlier from HS-7 been omitted? It appears as if an outlier remains in the data set. If HS-7 has been omitted, what is the additional point that appears to be an outlier?

In Figure 1, the data point from HS-7 has been omitted. None of the remaining points are outliers. Yet there is substantial scatter. Still, even with the observed scatter, the correlation coefficient is significantly different than zero, as discussed in the text.

Page 21: ***Sarcenski sample - Dan, were additional weathered areas observed in April 2004?***

I'd be interested in hearing the answer to this question.

Page 22, top of page: As stated in earlier comments, EPA would be unlikely to accept a Monte Carlo type of analysis of risk for this site.

Actually, I thought we discussed this and that I pointed out that there are many different ways that Monte Carlo can be used in an assessment (including, simply to help put uncertainties in perspective) and I thought that you indicated that EPA would keep an open mind on the issue.

Page 22, Section 5.1, 2nd paragraph: The emission models referenced were published for different applications. Stating that they are published does not add credibility for their use in this particular application.

I disagree. The statement is true. If there is a published dust emission model for a specific activity of interest, it can be used without modification for the precise purpose for which it is intended: to estimate airborne dust exposure for that particular application. The cited manuscript then simply demonstrates that these dust exposure estimates can be multiplied by measurements derived using the Superfund Method to predict airborne asbestos exposure with reasonable accuracy. That some of the dust emission models had to be modified in this study to address all of the activities of interest is a separate issue.

You should also note that I am not starting from scratch with my adaptations. I am starting with the models under the conditions for which they were validated and then indicating how they are modified in a rational way (accounting for the physics of the modifications) from the published models under the conditions in which they were validated. Therefore, knowing that these are models that have been developed for specific purposes and then describing the relationship between those purposes and somewhat different purposes to which they are being adapted seems to me to be important.

Even more important, several of the models are in fact being used in precisely the manner

in which they are intended (e.g. all of the construction models, the ATV model, which is just a vehicle, and the construction transport model). Then there are models that are being applied for a scenario that is of the same kind of activity for which the model is intended except that a few of the parameters are slightly out of the range over which they have been validated (e.g. the excavation model for gardening and child's play, the Copeland model for bicycling, and the tilling model for rototilling). Although adapting the Copeland model for walking and running was a little more extensive, I have tried to show that the adaptations are justified by the physics of the activities).

Page 23, first complete sentence: The emission models employed may be the best available models at this time for this application; however, there may still be a high degree of uncertainty associated with modeling emissions for walking, running, and biking. I question whether a vehicle emission model is applicable to a person or bike rider.

I agree that there is uncertainty associated with my adaptations. However, I also believe that the analysis that I went through to adapt the Copeland model for these activities (as described in detail in the text) is reasonable and I am comfortable that the manner in which I made the adaptations is likely conservative. Nevertheless, given the uncertainties involved, perhaps walking and/or running are among the best candidates for evaluation during the simulations. In fact, if such a simulation is conducted properly, we may be able to actually validate a new model for walking and running (which probably will not look too different than what I have proposed).

Note, with regard to bicycling, this activity is entirely analogous to the purpose for the Copeland model. It is a vehicle with wheels. I have simply modified some of the parameters because they are outside the range over which the Copeland model has been validated. Therefore, this application should be somewhat less uncertain than adaptations made to address walking and running.

Page 23, section 5.2, last paragraph: Referencing experience at other hazardous waste sites within the context of this document is irrelevant. Unless specific methods from other sites are referenced, then previous experience should not be referenced generically.

Per our discussion, I will use, "based on professional judgment."? I simply wanted to give the reader an indication of the fact that I have done hundreds of site risk assessments over my career and have developed a feel for the pathways that contribute most to residential exposure. If you have other pathways (such as the pathway for ATV activity), I would be happy to discuss whether they need to be considered. I just want to give people a feeling of closure.

Note that I did not initially include ATV riding for the simple reason that ATV riding is not typically conducted on a residential lot. However, the lots in this neighborhood are sufficiently large that such activity may be reasonable in this neighborhood

Page 25, 2nd full paragraph and Table 11: Please add the EPA 2002 reference to the list of reference. From context, I assume the reference should be EPA's Soil Screening Guidance, Dec. 2002, OSWER 9355.4-24. Also, I'm curious what modifications from EPA 2002 are employed since the text states that the only modification to the original AP-42 equation is the insertion of the Ra/d parameter.

Done.

Pages 25 - 29: The emission models described on these pages could not be exactly reproduced given the

references cited and the notes provided on the Tables. More explicit references should be given and an appendix added to show how the original models were adapted for use at this particular site.

Per our discussion, I am working to fix the omissions in the Tables 7 through 12. These are where the modifications are documented.

Page 27: This page presents a justification for use of the Copeland emission model to estimate emissions from walking, running, and bicycling. I question whether this model is valid at the much lower weights for these scenarios as compared with vehicles traveling on unpaved roads. Also, for the running and walking scenarios, these movements are not as fluid as wheels moving on a road so is there the possibility that a greater amount of dust would be kicked up from these activities as compared with vehicles?

Regarding bicycle riding, a bicycle is simply a vehicle that has two rather than four wheels. Therefore, other than adjusting for the relative "footprint" of the tire and for vehicle weight, the Copeland model is designed precisely to estimate emissions from such vehicles. Regarding the adjustments for the weight of the vehicle, while it is true that the weight of a bicycle and rider is outside the range over which the Copeland model has been validated, I recall that the documentation for the model indicates that, if anything, emissions are over-estimated for lower weight vehicles. If I have time, I will try to find the specific discussion, but I may not succeed in the near future.

Regarding walking and running, while it is true that one starts by placing feet on the ground with some impact, the velocity of a spinning tire is sufficient to cause each new portion of a tire to be placed on the ground with reasonable force. I believe that I discussed much of this in the text. Moreover, a tire produces dust continuously along its track. In contrast, a foot during walking produces dust during only about one third of its track (it is not in contact with the ground for the rest of the time) and this effect is even more extreme for running. Further, I believe that the extremely over-conservative manner in which dispersion is addressed in these models (as described in the text) more than makes up for any small difference in the force with which a foot is placed on the ground relative to a new section of wheel. Regardless, if there are questions about my adaptations, I believe the best way to settle the issue would be to include walking and running among the activities to be simulated.

Page 32, 2nd bullet: The moisture content of 0.2% was assumed for this site based on a value recommended by EPA's Soil Screening Guidance. A moisture content of 2% was assumed when subsurface soils were assumed to be disturbed. A geologist should be consulted to verify these values. Alternatively, moisture content could be measured during field activities conducted in 2004.

I have included references in the text of the document that refer to moisture levels measured at depth in piles, mines, and other places. I believe that these references suggest that an estimate of 2% for soil at depth is a reasonably conservative estimate. Also, as stated in the text, it would not be a big deal to include moisture content measurements as part of any future field work. In fact this was one of the recommendations of the document.

Page 33, first bullet: Provide a reference to the specific page from the Soil Screening Guidance that was used to estimate 90 wet days/year at the site. ***Is there a local meteorological station that could provide a more accurate value?***

The value was obtained from the map on P. 5-13 of the Guidance (EPA 2002). I do not know if there is a meteorological station that is sufficiently close to provide a better

estimate. However, the overall calculations are not going to be overly sensitive to small changes in the value of this parameter. This is a small effect.

Page 33, 4th bullet: A running pace of 8 mph for 2 hours seems excessive. This would result in running 16 miles per day, which far exceeds even a marathon training schedule. Given that the risks for this pathway are slightly elevated, consider modifying the running scenario to a more realistic scenario. The rates given for biking and walking appear to be more reasonable. ***Riding 4-wheelers was not specifically evaluated and residents have been known to ride off-road vehicles around the site. Should this pathway be added?***

I agree and have therefore lowered the mean velocity for running from 8 mph to 6 mph. I thought that this would be better than lowering the estimated duration and frequency for running. I have also lowered the number of days from 350 per year to 250 per year.

As previously indicated, I have added a scenario for ATV's.

Page 33, last bullet: The materials handling scenario may not represent the types of exposures that may be more likely to occur at the site. The residents consultant specifically asked about a utility worker scenario which would involve a smaller area. also, if new homes were to be constructed on the remaining lots, I would expect a smaller area of excavation but to greater depth, to install the home foundation. These comments are for consideration only. The modeled scenario should be protective of these exposures.

Because a utility worker scenario involves primarily excavation, loading, and dumping that would occur over a substantially shorter period of time than assumed for construction, this scenario is addressed by default. Also, a remediation scenario was added to address construction in burial piles.

Page 34, 2nd bullet: The Oregon DEQ and Region 10 default body weight for adults is 70 kg. Although the difference in value is minimal, please update calculations to include a 70 kg body weight for adults.

The difference is tiny but I will try to make the change, if it is not difficult. 160 lbs works out to 73 kg (a conservative difference of 4%). In the scheme of things, this is nothing. I am worried about spending a lot of time on trivial changes.

Page 34, 5th bullet: Although the assumption of 50% on bare ground may be conservative, I'm curious whether the shoulders of roads near the site are dusty and if so whether asbestos may be present along roads. Dan, do you remember the condition of roads/shoulders? This comment may have minimal impact on the assessment.

I would be interested in hearing Dan's observations on this.

Page 35, first bullet: Provide additional information on the reference for the dispersion factor (Appendix D, from EPA 2002).

I added the change is indicated.

Page 35, 3rd bullet: I think the length of the haul road would be shorter as most of the undeveloped road sites are adjacent to or near existing paved roads.

I agree, but since this will not affect conclusions in the least and since the current case is

conservative, I have not made the change.

Page 35: For the scenario described on this page, what is the construction scenario being modeled? Construction of new homes? Remediation of the site? Please provide additional information to place this scenario into context for the risk assessment.

I have modeled the component activities of construction projects that are the major contributors to dust emissions. These include, bulldozer excavation, grading, loading and dumping (bulk excavation), and transport on unpaved surfaces. Importantly, I have modeled each of these separately assuming that they are conducted for 8 hrs/day, 250 days/year for one entire year.

For worker exposures, it is not necessary to add the individual contributions because workers are assumed so close to the sources that contributions from more remote activities are unimportant. For residents, I have summed dust contributions from each activity so that it mimics a project in which one of each kind of equipment are continuously working simultaneously. In the case of residents, I used the dispersion factor recommended in EPA 2002, because residents are offsite and thus should be receiving exposures that are averaged over the project area. In both cases, the asbestos concentration assumed for the source concentrations is the maximum of the observed composite samples with contributions from the embedded ACM included. This should be conservative for any work averaged over a large area of the site (which is appropriate for construction activities).

Given the above, the scenarios involving residential exposure during construction should be conservative for virtually any kind of construction activities that may occur at the site. It should adequately address any long-term project that would involve construction over a substantial portion of the site. It should also be conservative for smaller, short-term projects (including remediation) because, even if such projects were to be concentrated in hot spot areas (such as for remediation), the limited area of the hot spots would mean that such projects would not last long and the dust generating phase would need to last longer than a month before construction in the worse of the hot spot areas would contribute more to exposure than is currently estimated using the current scenarios. In fact, even if one were to assume that continuous construction activities were conducted for an entire year in the hot spot that exhibited the single greatest asbestos concentrations with no dust control, risks to the surrounding residents would still not exceed $1x10^{-5}$.

Note, I have now added a second scenario that addresses remediation related risks in hot spots directly so that my statements above can be verified.

Page 36, Section 5.3.3, second equation: The term 5 m should be 3 m for wcp as given in Table 13, unless there is a discrepancy between the text and Table 13. Please resolve.

This error will be corrected. The calculations and the descriptive text agree, it is the table of input values and the values in the written equation that needs to be corrected. Everything will be made consistent.

Page 37, 2nd equation: Please include a description of the assumptions for hours in a lifetime. Was a lifetime assumed to be 70 years, which is the standard EPA default value, or some other length of time?

For these calculations (the results of which are presented in Table 15), a lifetime

represents 24 hrs/day, 365 days/yr for 70 years. This is used as the denominator for calculating the fraction of a lifetime in Column 6 of Table 15.

Page 37, last paragraph: The EPA 2001 reference cited in the last paragraph does not need an "a" unless there's an additional EPA 2001 reference which was unintentionally excluded from the list of references.

This will be corrected.

Page 38, first full paragraph: I disagree with the assumption that children younger than 3 are unlikely to have regular access to ACM. In fact, ACM is scattered across most of the residential lots at the NRE site and as such, even toddlers could access this material.

We are probably thinking about this differently. I am talking about toddlers who barely walk. Such children are not typically left unattended to wonder around a yard. I will change the text to say younger than 2, which should resolve this perceptual problem. It will not affect the calculations.

page 38, equation: Please provide units for the numbers provided in the equation. Also, what is the source of the value 0.76? This value should have been the instantaneous dust concentration for walking, which is 1.3E+00 mg/m3 in Table 14.

By correcting wcp to 3 from 5 (as indicated in your comment concerning Page 36, Section 5.3.3), all of the numbers agree. I will make the corrections.

Page 40, 4th paragraph: EPA would argue that in fact, hot spot areas may be present throughout undeveloped portions of the site. Because the work that EPA conducted during 2003 was part of an Emergency Response, efforts were primarily focused on areas adjacent to occupied residences. Given the findings at some of these locations, we anticipate that additional burial piles could be found in other areas of the site. As a result, for construction scenarios, perhaps data from hot spot samples should be evaluated. Additional information collected from undeveloped properties during 2004 may be used to assess this scenario.

I will include a scenario involving a hypothetical 2-month cleanup action in hot spots and will assume that highest concentration observed among the hot-spots as a conservative input assumption. Regarding other data, we can incorporate such other data in the final risk assessment.

Page 41, Section 5.4.1, 2nd paragraph: EPA notes that in most risk assessments, the slope factor indicates the likelihood of contracting cancer, not *death* from cancer. This appears to be unique to the proposed asbestos unit risk factor. Also, in this paragraph, the analytical method should be corrected to "ISO Method 10312."

Just a note that it is not just asbestos: the slope factors for any chemical in which the factor was determined from epidemiology studies will be based on deaths from cancer. It is only slope factors derived from animal studies that are based on incidence of cancer.

Method correction Done

Page 42: In the equation for Rpop given in the middle of this page, what do the factors 0.786 and 0.214 represent?

These are simply the coefficients representing the fraction of nonsmokers and smokers, respectively in the U.S. I will add a note for the equation to make this clear.

Page 43, 3rd paragraph: this paragraph includes an excellent description of the limitations of the proposed URF and the need for analytical methods that can discriminate between "protocol" and other structures.

Thanks.

Page 44, Section 5.5.1, 2nd paragraph: Although surface removal of ACM occurred over much of the site in 2003, remediation did not occur in all areas. Undeveloped properties were not investigated or assessed to any real extent. Further, ACM has reemerged in 2004, demonstrating that surficial removal is not adequate to control long-term exposure to ACM. Apparently, a new layer of ACM may surface annual in some areas. Assumptions about the absence of ACM on the surface should be qualified as large amounts of ACM may remain beneath the surface and rise up over time. Further, risks associated with walking, running, bicycling and rototilling should be recalculated using soils containing ACM to more accurately assess potential risks. I did this quickly, and in most cases, based on the maximum composite concentration of asbestos and ACM, risks exceeded 1E-04 for both protocol and 7402 structures.

I have done all of the recalculations with the new assumptions from all of your comments incorporated. You can see that, with all of the modifications incorporated, only the ATV scenario leads to risks that exceed one in ten thousand (among residential scenarios) and even for this scenario, it is by less than a factor of two.

Page 44, Section 5.5.1, final paragraph: If risks are 20 times greater than those estimated in Table 19, then many scenarios would in fact exceed both the Oregon DEQ and EPA acceptable risk levels. Given the previous comment about ongoing and visible ACM migration to the surface, then the likelihood of activities occurring on areas containing ACM may exceed EPA's risk range.

Given the new, recalculated risks, this paragraph will be rewritten.

Page 45, first complete sentence on page: Considering changing this statement given the recalculation of risks using soils with ACM.

This statement will be modified to reflect the new re-calculated risks.

Page 45, first full paragraph: Although people that currently reside at the site are educated about the ACM that may be present on their properties, a baseline risk assessment should not assume that people would avoid these areas. Also, as time passes, new families may move to the area and be less informed about potential risks. Assuming that people excavate in hot spots is not unreasonable, given that some excavations in such areas have already occurred at the site (e.g., excavation into a hillside to put in a deck resulted in exposure of large amounts of ACM).

This paragraph will be modified. However, it addresses primarily gardening and play, which are two activities that are unlikely to be conducted in hot spots (areas of high concentrations of ACM) simply because it is so difficult to dig into such materials using simple hand tools. Actually, the new calculations now take into account the highest concentrations observed at hot spots.

Page 45, last paragraph: Comparison of EPA's sample results to Berman's should be interesting. First, if additional amphiboles are identified in EPA's samples, then a more careful, quantitative analysis of amphibole asbestos may be warranted. Second, EPA's glove box results may be useful to "truth" some of the emissions modeling presented in this report. Third, comparison of our elutriator results (for soils

collected around residences) to the current report may indicate variability in concentrations and fiber releases across the site. Also, when the site was surveyed in April 2004, steam pipe wrap was identified at several properties. Thus, exposures to amphibole may be more likely than indicated in the current report.

I agree that results will be interesting compare. Also, see answers to the previous Comment Page 5, first bullet (regarding the glove box).

Page 46, first paragraph: Consider modifying statements about percent overall exposures to amphibole in light of material observed on site in 2004. Additional data collected can be used to update the assessment.

The statement will be modified. However, it still is apparent that the chance of encountering amphiboles at the site is limited to small areas so that the overall probability is small. Actually, I have now incorporated a section that formally addresses amphibole risk.

Page 46, Section 5.5.2, 2nd paragraph: Assuming that workers will perform dust control activities should not be a consideration in a baseline human health risk assessment. Rather, the need for dust control may be an institutional control placed on the site if future development occurs. A means of ensuring that required techniques are implemented is necessary.

See answer to Comment: Page 1, 6th and 7th bullet.

Page 49, first paragraph: Performing any type of quantitative estimate of uncertainty is inappropriate at this time, especially considering the large uncertainties associated with the emissions modeling and also given the comments on the current report.

- ***I have reviewed and modified this section in an attempt to address most of the issues raised. However, a section evaluating uncertainty along the lines of what I have presented is a common and typically required component of a risk assessment so I have chosen to leave it in. It is helpful for putting the conclusions from risk assessment in proper perspective. I think you will also agree that it is now carefully and properly balanced.***

Page 49, 3rd paragraph and page 50, 7th bullet: ***EPA may consider abrading ACM in a glove box and measuring fiber releases from this activity....***

While I am confident that one will be able to see asbestos emitted from abrading ACM in a glove box, I am not sure that experiments in the glove box will tell us much more than that. Certainly, there will be no easy way to convert the concentrations that may be observed in a glove box to something that relates meaningfully to exposure concentrations that might obtain in the real world. See my response to Comment Page 5, first bullet.

Page 49, Section 5.7: The additional data evaluated by EPA using both the glove box and the elutriator may be useful for understanding the relationship between fiber releases from soil and observed ACM.

I look forward to evaluating and comparing these data with the data that we generated.

Page 50, 5th bullet: See previous comments about dust suppression activities. Risks associated with construction activities should not be tempered by assumptions regarding dust control activities. It is not routine to consider institutional controls in a baseline risk assessment.

See my response to Comment Page 1, 6th and 7th bullets.

Page 51, 2nd bullet: Additional investigation of the overall site should be completed to ascertain whether more than 9 additional hot spots require consideration. As mentioned earlier, EPA did not focus on undeveloped properties. Also, section 2 specifically mentions 7 hot spots - where are the two additional hot spots described in this bullet?

Given the manner in which the risk assessment is constructed, the results can be adapted to the new areas as they are evaluated. General statements have also been added to address the need for more sampling and analysis. I am not sure that I need to be more specific than I have been in the revised document. In fact, given that the number of burial piles and such is currently a moving target, it is perhaps better not to be too specific. In any case, any new data that are developed will be incorporated into the final risk assessment to be developed for the site.

Table 3: The column headers in this table have typos - "strictires" should be "structures."

The error was corrected, thanks.

Table 9: The speed of the rototiller seems a bit fast, given that 4 mph is a brisk walking pace. Most people using a rototiller are walking slowly and may actually stop when tilling becomes difficult. To determine the time required to till an acre, what is the source value in the denominator?

As indicated in the revised tables, the speed of the rototiller has been reduced to 2 mph. Also, in the second to last line of the calculation to determine the time required to till an acre, I divide the linear number of feet by the speed of the rototiller (converted to ft/s). Which provides an estimate of the number of seconds required to till an acre. The reciprocal of this value is then the number of acres per second. I believe the revised table makes this all clear.

Table 11: EPA 1989 is not provided in the references. What is the source of the model used in this table?

EPA 1989 has been added to the references and it is the source of the model.

Table 13: Several discrepancies were noted in this table as compared with the text and with values derived from formulae presented in prior tables. To obtain the results for emissions provided in Table 14, the following additions/changes need to be made in Table 13:

- A particle size multiplier of 0.36 (or 0.35) should be added for the walking, running, and biking scenarios.
- The moisture content for "Transport" was listed as 2; however, 0.2 was used in calculating the emission rate.
- The wind velocity for Child-play/gardening and Handling ACM should be 3 m/s (not 1.5 m/s).
- The fraction of time on bare ground for walking, running and bicycling should be 0.5.
- As an explanation for No. of vehicle wheels, include a reference to Appendix C in the comment. Why is the No. of vehicle wheels for a bicycle given as 0.19, when Appendix C indicates 0.17 for an off-road bike?
- For silt content, include a reference to Appendix D.

These corrections have been made.

Note that the value of 0.19 for the number of bicycle wheels was a typographical error.

Dr. Berman and I have discussed these issues; however, for the benefit of our partners and to document the discrepancy, I have included the comment.

Appendix B: Please make the following correction...4.7/(1 + 4.7) = 82%. Also, please provide the section in Berman and Crump (2001) which addresses how shorter structures are accounted for in epidemiology studies. In the final report, will a copy of the raw data sheets from TEM analyses be provided to EPA?

We will provide copies of all of the raw data sheets. We just have to put them together. Regarding your question concerning Berman and Crump (2001), much of the document deals with the relative potency of short structures. In Chapter 7, for example, the relative potency of short and long structures are evaluated qualitatively based on animal data. Specifically in Section 7.4.3 the published study (Berman et al. 1995) is discussed. In this paper, we showed quantitatively that the best estimate of potency for short structures (toward lung cancer) is zero and also provided some upper bound estimates. In Chapter 6, much of the chapter is devoted to applying our new exposure index in which (among other things) structures less than 5 um are considered non-potent) to the epidemiology database and conclude that using our improved exposure index, better reconciles the existing epidemiology studies (i.e. provides improved agreement across studies) than the current EPA exposure index.

Perhaps most important is the following.... As shown in the background document to the air method (Berman and Chatfield 1990), all distributions of asbestos that have been observed in air contain primarily short structures. In most cases, the fraction of structures longer than 5 um constitute no more than 20% of the total and frequently constitute even less. Thus, short structures were certainly present in large numbers in the exposures to which cohorts studied in the published epidemiology studies were exposed. Further, any potential contributions from these structures were certainly included in the observed mortality from these studies, even though the short structures themselves were NOT included in the exposure estimates. Thus, to the extent that cancer risks are estimated based on risk factors derived from these studies, the effects of short structures are addressed by default, even if they are not counted as part of the exposure estimate.

Note that this idea is presented both in Berman and Crump 2001 and in USEPA 1985 (the Health Effects Assessment Update), but I have not been able to find the quotes quickly.

Table C-1: A child old enough to ride a bicycle is likely to weigh more than 30 lbs. The 50th percentile weight for a 5-year old (approximate age for kids riding two-wheeled bikes) is approximately 40 lbs for boys and girls (National Center for Health Statistics, 2000 Growth Chart, www.cdc.gov). Given that the bicycling scenario is assumed to occur for 2 hours per day, every day, then an older child may be appropriate to evaluate for this scenario. Also, how do the values presented in Table C-1 and C-2 relate to the values used in Table 13 to model dust generation from bicycling. Footnote d contains a typo in the word "walking." In footnote g, the human foot dimensions seem rather long and thin to me, for an average foot.

Note that, although I provided an estimate of the weight of a child who might ride a bicycle, this estimate was not included in any of the risk calculations. Only adult weights were assumed. Therefore, this concern is not relevant to the conclusions of the report.

Appendix D: Table 13 indicates that 35% was assumed for silt content based on the upper end of the values measured on site. Was a statistical analysis performed for measured values? If a 95th percentile

was performed then the value likely would be higher than 35%.

Even though this correction is very small (only about 10%), I have changed the estimate of silt content to the maximum value observed (38%).

Appendix E, page 87, 2nd to last paragraph: The mixing height of 1.75m may be appropriate for adults, but a relatively high number of children reside on site, and in many cases they are quite young. Especially since concentrations tend to be much higher closer to the ground, an additional scenario should be considered for children using a lower mixing height. In the subsequent section on gardening and children playing, additional comment about the conservatism associated with a mixing height of 0.5m for young children should be included.

Note that the scenarios for running, walking, and bicycling assume fairly strenuous conditions that are unlikely to be achievable by a young child. Children walk, run, or bicycle more slowly and (lacking the stamina of an adult) for shorter time periods than adults. Moreover, over a 30 year period, all individuals will spend approximately half of that time as an adult (at least in size). Given these two considerations, it seems reasonable to assume an adult mixing height for these three scenarios.

Regarding gardening and child play, the size of a child's reach was considered explicitly. I will see if an additional comment concerning this conservatism should be indicated.